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Taking Money Seriously

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by

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ABSTRACT

B1 - Laidler, David E. --

B2 - Taking Money Seriously

C2 - This survey paper argues that Walrasian markets and monetary exchange are alternative, not complementary, arrangements for co-ordinating economic activity; that if we realise this, many informational anomalies and price rigidities which appear "irrational" in a Walrasian context can be understood as the outcome of maximising behaviour in a monetary economy; and that the precautionary approach to modelling the demand for money takes on a particular importance in this way of looking at things. Empirical evidence on the demand for money is cited in support of this point of view, which tends to re-instate the quantity of money as an important economic variable, in contrast to the New-Classical vision of the economy which downgrades money's importance.

B6 - *Canadian Journal Economics*

B7 -

B8 -

C4 -

B4 - University of Western Ontario

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ERRATUM

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The label in chart 1b should read

$$\hat{y} = \underset{(0.13)}{-0.4} + \underset{(0.02)}{0.15}\,\hat{m}$$

INTRODUCTION

There are many parallels between the "Keynesian Revolution" and the "Monetarist Counter-Revolution", but surely the strangest of them is this: both were set in motion by economists who were convinced of the central importance of money to the economic processes they were investigating, and both ended up evolving views of the world in which money has but a minor role to play. This parallel did not arise entirely fortuitously. Whatever Keynes may have intended, what came to be called Keynesian economics soom came to rely on a version of Walrasian general equilibrium analysis in which market mechanisms exist and function independently of the behaviour of the supply and demand for money; and monetarism took over the same theoretical foundations.[1]

In this lecture I shall argue three points: that such Walrasian analysis does not permit money to be taken seriously; that if we do take money seriously in the sense of regarding monetary exchange as an alternative, instead of a supplement, to Walrasian mechanisms for co-ordinating economic activity, many of the apparently anomalous rigidities upon which predictions about money's importance appear to hinge can be understood as the outcome of maximising behaviour; and that if we also take money seriously in the sense of paying attention to the empirical evidence we have on the nature of the economy's demand for money function, we find that some of it does indeed point to the desirability of looking at monetary exchange in just this way.

MONEY IN KEYNESIAN, MONETARIST AND NEW-CLASSICAL ECONOMICS

In the economics of Keynes, as in Classical economics, money was a means of exchange; and textbook macroeconomics even now refers to "transactions" and "precautionary" motives for holding money, which are said to derive directly from that role. However, when monetary economists adopted Walrasian general equilibrium, in its IS-LM disguise, as their basic vision of economic activity, they adopted a model which could not generate such motives internally. Whatever the vocabulary of the monetary economics that developed after the publication of the *General Theory*, its logical structure came to treat money as an asset pure and simple; and the emphasis which Hicks (1935) and Keynes (1930, 1936) placed on money's role as a short term speculative asset represented an important contribution to monetary economics precisely because it laid the foundations for a theory of the demand for money as a store of value, and because such a conception of money was the only one which could be accommodated comfortably within a Walrasian frame of reference.

The monetarist counter-revolution was an attempt to change perceptions about empirical phenomena, not to create a new theoretical structure. It therefore did nothing to interrupt the process, already close to completion in the 1950s, of integrating monetary theory with Walrasian value theory.[2] The further this process was pushed, the more did the representative model of a monetary economy come to resemble one of a barter economy in which there happened to exist a peculiar asset called "money" whose "real" (ie. "utility" yielding) quantity varied in inverse proportion to its price in terms of goods. The utility in question was said to arise from money's role as a means of exchange, of course, but there was no such role for it to play within the logical structure of the representative macroeconomic model. Though that

model was undoubtedly useful as a starting point for empirical work, it was also very vulnerable to theoretical criticisms such as I have been summarising here. Clower (eg. 1965, 1967), Hahn (eg. 1965) and Tsiang (eg. 1966), for example, had all articulated them by the mid 1960s, but though the end of that decade saw much interest in the micro-economic foundations of monetary economics, this particular line of argument was to become, and indeed remain, something of a minority taste among macroeconomists. The reason for this was the success of new-Classical economics.

The original aim of Robert E. Lucas and his collaborators was to provide a micro-foundation for monetarist predictions about the short-run non-neutrality of money, and Friedman's (1968) long run "natural unemployment rate" hypothesis. The nature of that endeavour initially attracted attention to the supply side of the economy in general and the labour market in particular; only later, (though surely inevitably) were new-Classical economists forced to face up to the fact that the Walrasian framework with which they had continued to work does not provide an internally coherent justification for the existence of money. Their resolution to this difficulty has taken two forms. Some, the proponents of what Hall (1982) has called the "new monetary economics", have pushed the money as a store of value view to new limits, and have concluded (correctly as far as the world described by their model is concerned) that, if money is dominated in rate of return (as it is in the real world) no one would hold it unless the government compelled them to do so.[3] Others, notably Lucas (eg. 1984), have reintroduced money's means of exchange role into a dynamic, but still essentially Walrasian, model in the shape of an arbitrary "cash in advance" constraint which derives

directly from the work of Clower and Tsiang (though the rescue of Walrasian style macroeconomics was hardly what the latter intended that idea to accomplish).

Protagonists of the approaches in question would no doubt argue that they are still exploring matters of logic, and that their theories should not yet be expected to meet the test of empirical relevance. Even so, the widespread adoption of these approaches has opened up a yawning gap between theoretical and applied work on monetary issues. In either type of new-Classical model, monetary shocks can cause price level fluctuations, but their assumptions of competitive equilibrium and rational expectations ensure that the amount of real harm which results is trivial. Setting aside issues of superneutrality, it is the fundamental prediction of new-Classical theory that only unanticipated changes in the money supply have consequences for real variables. Given the amount of information available in the real world about the behaviour of monetary aggregates, and the speed with which it becomes public, it is impossible to account for the magnitude of observed cyclical fluctuations in real income and employment in such terms. That is one reason why new-Classical economics, mainly at the prompting of Kydland and Prescott (1982), is in the process of spawning "real business cycle theory" as an alternative, completely non-monetary, explanation of economic fluctuations.[4]

"Keynesian" arguments about the minor relevance of money for the behaviour of prices, output, and employment were, of course, very different from those of new-Classical economics. They involved assertions about the existence of liquidity and investment traps, and of sociological influences on money wages and prices so strong as to swamp the effects of conventional market forces. Monetarism rebutted these arguments with empirical evidence

about particular functional relationships, and also about broader based associations among the quantity of money, the price level, and output. Such evidence continues to accumulate, and those of us concerned with applied problems in general, and policy issues in particular, remain convinced that, in the real world, "money matters" as much as ever.

To support this proposition, let me present two scatter diagrams of data taken from the FRB St. Louis *International Economic Conditions (Annual Edition)* (June 1987). Figure 1a plots the relationship between average inflation rates ($\dot{p}$) and average nominal-money-less-real-income growth rates ($\dot{m}-\dot{y}$) for 11 countries over the period (roughly) 1970-85. Figure 1b plots the year by year relationship, for each of those same countries, between the deviation over a two year period of real income growth from its long run average value ($\hat{y}$) and the deviation in the first of those years of nominal money growth from the average value prevailing over the previous three years ($\hat{m}$). These scatters reveal two empirical relationships central to monetarist doctrine, namely a long run one between money growth and inflation, and a short run one between changes in money growth and subsequent real income growth.[5]

The inflation money growth relationship appears to be the stronger of the two, but figure 1b nevertheless indicates that changes in money growth rates significantly increase the probability of subsequent variations in real income growth. Brunner and Meltzer (1987) have recently recanted their view that monetary shocks are the dominant impulse driving economic fluctuations in the light of their reading of the evidence generated by the 1970s and 80s, and are now more inclined to take an eclectic view of the mechanisms underlying the cycle such as has long been advocated by Leijonhufvud (eg. 1986). The

data I present here certainly leave room for a substantial non-monetary component among the factors driving the cycle, but they are also consistent with the view that, if not dominant, monetary impulses have nevertheless remained systematically important over the very period in which popular opinion would have it that monetarism was being discredited.

I do not present these data as "proving" anything about the real world. Rather my intention is show that the 1970s and 80s have generated no crucial empirical experiment that would force an impartial observer to abandon monetarist doctrine.[6] The rise of new-Classical economics has been based on theoretical arguments, not empirical, and it is surely ironical that a body of doctrine, whose immediate motivation was to provide a tight theoretical rationale for monetarist generalisations about the short-run empirical importance of money for real variables, has ended up re-establishing the Classical doctrine of "Neutral Money" on firmer theoretical foundations than ever. As I shall now go on to argue, new-Classical economics, despite its great theoretical clarity and rigour (I judge it here by the standards of macroeconomics), is at best unhelpful, or at worst downright misleading, in empirical applications because its models, even those which employ a cash-in-advance constraint, do not encompass the basic facts of economic life which require economies to make use of a means of exchange in the first place. They do not take money seriously from a theoretical point of view, and that is why they cannot take it seriously empirically.

MONEY AND ALTERNATIVE MARKET MECHANISMS

New-Classical concerns with the micro-foundations of macroeconomics did not arise in an intellectual vacuum. Lucas's (1972) paper grew out of his earlier contribution (with Leonard Rapping) to the celebrated (1969) Phelps et al. volume on *The Microfoundations of Employment and Inflation Theory*. Moreover, the late 1960s and early 70s saw widespread efforts to find a satisfactory micro-foundation for the theory of money *per se*. I have already alluded to the work of Clower, Hahn, and Tsiang, and mention ought to be made too of the contributions of Hicks (1967), Leijonhufvud (1968), Niehans (1971), Brunner and Meltzer (1971), Ostroy (1973), Melitz (1974), Howitt (1974), Goodhart (1975), Jones (1976), and Alchian (1977) among others. Certain common themes ran through this otherwise heterogeneous collection of work. All of it drew attention to the enormous costs of generating and processing the information needed to co-ordinate the activities of individual agents in that complicated set of social arrangements we call an economy, and all of it argued that the institution of monetary exchange should be understood as a device to deal with them.[7] Such ideas have many antecedents in Classical economics, because, for two hundred years at least, it was recognised that a barter economy would be so costly to operate that it could hardly be expected to exist outside of the economist's imagination, except in extremely primitive (at least in material terms) societies.

Within the Walrasian economy from which the contributors to the above-mentioned literature were trying to escape, information and incentives sufficient to co-ordinate the activities of otherwise isolated and self-interested agents are provided by the structure of relative prices. These have to be equilibrium prices because only in their presence can agents

come to market secure in the knowledge that what they bring with them will be just sufficient to purchase what they wish to take away. Furthermore, if we assume that supply and demand for every good and service always match, perhaps it is also harmless to assume (it is certainly usual) that the Walrasian market is a place where agents can costlessly find willing trading partners as well. Now the information and co-ordination problems which the "market" solves in this Walrasian story include those with which "money" deals in traditional accounts which begin with the inconvenience of barter. That is why monetary theory based on Walrasian general equilibrium analysis either treats money as an asset pure and simple, or has to introduce monetary exchange in an apparently arbitrary fashion by appealing to a cash in advance constraint. It is also why the lines of enquiry opened up in the literature to which I have referred above still deserve the attention of monetary economists.

The Walrasian auctioneer solves the problem of co-ordinating economic activity by performing three distinct tasks: setting market clearing prices, informing agents about them, and bringing suppliers and demanders together so that trades can go through. In contrast, the adoption of a common means of exchange in an initially non-Walrasian barter economy simplifies co-ordination problems but but does not eliminate them. Long transactions chains are ruled out if agents with something to sell take money in exchange, and, when it comes to buying, offer money. It takes two parties to strike a single bargain, however; and even in a monetary economy, agents offering specific goods or services for sale still have to find someone who wants to buy them, agents offering money in exchange for specific goods or services still have to find sellers, and buyers and sellers have to find a mutually acceptable

price. Some degree of market uncertainty, and associated search and transactions costs, which would be completely absent from a Walrasian set up, therefore remain in money-using economies.

Some contributors to the literature to which I referred earlier, notably Howitt (1974, 1979), have found it helpful, and realistic, to note the existence of specialised agents, dealers or middlemen, who play a key role in coping with these remaining co-ordination problems. These dealers (who may or may not also be producers) act as specialised traders, located at specific places and open for business at specific times. They hold inventories of goods ready for sale, and set their prices too. Consumers are thought of as coming to these dealers' places of business during working hours, purchasing goods from them, and (if the dealers are also producers) as selling factor services to them as well. Monetary exchange, in the view of market mechanisms to which I here refer, is an institution which naturally complements the dealer. If sellers are specialised in the sense of offering for sale a well defined class of goods, then it is clearly sensible to think of them being willing to take in return a commonly acceptable means of exchange, and as posting their prices in terms of that means of exchange too. Moreover dealers must buy goods to replenish their inventories, and producers must pay the suppliers of their inputs. It is also sensible to expect that these trades will be mediated by money.

Now note that this vision of the trading process amplifies traditional accounts of how monetary exchange deals with the inconveniences of barter by cutting down on long transactions chains and saving "shopping time". In a dealer economy, a consumer wanting something specific does not have to search the market for some other agent who has it for sale. It will suffice to go to

the right dealer. Money does little to save the consumer's shopping time in this view of the world. Its main function is to simplify the act of exchange *per se* (though of course the combined effect of dealer mediated trade carried on with money is to reduce the many inconveniences of barter). This point is of some theoretical importance, because it is possible to conceive in principle of an economy which replaces the Walrasian auctioneer with dealers and monetary exchange in one of his three roles, that of bringing buyers and sellers together, but maintains that entity to set market clearing prices and to keep agents informed of them.[8] Furthermore, if the auctioneer's capacity for conveying information to agents about market clearing prices is limited to telling them about their selling prices, then such a set-up takes on a familiar new-Classical aspect, becoming in effect Lucas's version of a cash-in-advance economy.

A model of this type, though, is unusually artificial, even by the standards of macroeconomics. Dealers are not just a useful fiction like the auctioneer, they exist; and it is awkward to work with a system which relies on both entities simultaneously, one to justify the existence of money and the other to set prices and inform agents about them. Once they are introduced, it is natural to have dealers set prices, and also, along with their customers, collect and process the data on which market activities are premissed. If prices are set by individual dealers, however, they may differ for the same goods at different places in the economy. Though consumers may not have to spend time looking for suppliers of what it is they want, they will nevertheless find that it pays to search for a favourable price. Dealers too face problems which would not arise in a simpler world. Presumably they wish to maintain their prices at market clearing values, but they must devote

resources to calculating those prices. Sales volume will convey some information about whether prices are right or not, but if consumers are shopping around, it will have a stochastic character. Price setters who start off with the right price must therefore distinguish between random fluctuations in sales and those which signal a lasting shift in demand, and respond only to the latter.

All this gives money a more complicated role to play than it has in a simple cash-in-advance economy. If consumers find it worthwhile to shop around for favourable prices, the timing of their transactions, as well as those of dealers, becomes stochastic. They will therefore find it convenient to hold inventories of goods to smooth out discrepencies between the timing of acts of consumption and purchase. Dealers too will need inventories of goods in stock to absorb discrepencies between sales of goods and purchases from suppliers, whether these arise from random sources or from the possibility that it takes time to solve signal extraction problems before prices are changed. Where trade is carried on by money, however, movements of goods inventories must involve complementary fluctuations in the pattern of cash inflows and outflows, and these in turn must be absorbed by an inventory of cash balances, (or, depending upon the sophistication of the economy and the costs of trading in financial markets, of liquid assets). In short if we dispense with the auctioneer entirely and have prices set endogenously, we create a world in which a precautionary demand for money becomes of the essence.[9]

THE PRECAUTIONARY DEMAND FOR MONEY AND PRICE STICKINESS

The traditional textbook model of the precautionary demand for money begins with individual agents who inhabit an economy much like that sketched out above. The model takes it for granted that transactions are mediated by money, and that, because of market uncertainty, agents face a stochastic pattern of cash inflows and outflows. It also takes the existence of other financial assets for granted, and argues that, if there are costs of transforming other assets into cash, agents not only use money as a means of exchange, but also hold a "buffer-stock" of cash to reduce the frequency with which they incur the costs of trading other assets for means of exchange in order to permit their transactions in markets in goods and services to go through. Such a model leads to a conventional formulation of the "long run", demand for money function, in which the demand for real balances rises with some scale variable measuring the real volume of market activity and falls as the opportunity cost of holding money rises.[10]

Now it is of the very essence of a demand for precautionary balances that, in the individual experiment, its value should fluctuate around an average or target level as the agent encounters those unpredicted fluctuations in the pattern of payments and receipts against which the balance is held in the first place; and such an approach to modelling the demand for money also enables us to predict the existence, in the market experiment, of a reduced (or quasi-reduced) form equation describing the short run behaviour of real balances which displays the key characteristic of what we call (misleadingly if this explanation of the phenomenon is correct) the "short run demand for money function", namely a lagged dependent variable on its right hand side, or, more generally, strongly serially correlated residuals. However, as I

have shown elsewhere (Laidler 1988), a necessary (not sufficient) condition for this result to arise in the context of a "buffer-stock" model of the demand for money is that there exist a degree of price stickiness in the system; and, of course, the existence of price stickiness also produces just the kind of short-run non-neutrality which, I have argued above, provides a strong empirical incentive to take money seriously.

It is a commonplace that price stickiness can arise, quite mechanically, from inertia in expectations, from the existence of nominal contracts of significant length, or indeed from price setting agents facing non-trivial costs of changing them; it is also a commonplace that the twin assumptions of Walrasian equilibrium and rational expectations which underpin modern new-Classical monetary theory rule out this phenomenon. However, if we take the proposition that monetary exchange reduces, but does not eliminate, uncertainty from market transactions, we are not only led to postulate the precautionary model of money holding referred to above. We are also led to take a more benign view of price stickiness assumptions, whether we motivate them in terms of systematic expectational errors or institutional rigidities. Precautionary money holdings and price stickiness both arise from the same source, namely the informational imperfections which characterise an economy in which activity is co-ordinated by monetary exchange.

Consider in this context the rational expectations idea. The proposition that maximising agents will make full use of all information economically available to them is irresistable, but how much information is that? The assumptions about the costs of information underlying the new-Classical case of rational expectations are in fact very special indeed. The marginal cost of generating relevant information rises in one step from

zero to infinity. Information and computing power are either free, or completely unavailable, and scarcity of information occurs because the marginal cost of acquiring it becomes infinite before some theoretically ideal maximum amount of information (that which would guarantee the achievement of full Walrasian equilibrium) is reached, and not because more information is to be had at a finite price. One can easily accept the rational expectations idea while simultaneously denying the relevance of this special case, and from a more general viewpoint, that idea does no more than compel us to think of agents gathering and utilising information up to the point at which the marginal cost to them of acquiring it equals the marginal benefit which its possession confers.[11]

In a money using economy, the marginal benefit from acquiring information must be the reduction in transactions costs which agents achieve by being better informed. Such a reduction will be achieved by a closer matching of cash receipts and cash outlays so as to avoid the need to engage in costly trade in financial assets; but, as we have seen, the precautionary demand for money arises precisely from its capacity to reduce those same costs. If information enabling agents better to match up their cash inflows and outflows is to be had at zero marginal cost, they will of course acquire it, use it, and hold a smaller precautionary balance than they otherwise would; but if it comes only at rising marginal cost, and if money holding is a cheap buffer against the consequences of ignorance, those agents' decisions will, as a matter of rational choice, be based on less information than the economist looking into the economy from the outside would regard as available to them. Moreover, as Galbraith (1988) has recently shown, such "economically rational" expectations are capable of generating serially correlated errors which agents do not find it worthwhile to eliminate.

Information gathering and money holding do not exhaust the avenues open to agents wishing to reduce the costs imposed upon them by the existence of a stochastic element in the time pattern of their payments and receipts. The price at which they stand ready to deal is a variable under agents' control, and the effects on their cash flow of fluctuations in demand and supply which they encounter may be reduced by varying it. However, haggling over prices in and of itself takes time and trouble which can be avoided if agents enter into contracts with one another to deal at preset prices, or, in the absence of continuing relationships among specific agents, if sellers simply state prices at which they stand ready to deal over non-trivial time intervals. Price flexibility in goods markets, then, may be viewed as a means of reducing transactions costs in asset markets, albeit a costly one. The degree of price flexibility in an economy should be higher, the higher are the costs of its substitutes, namely information and money holding; and the existence of precautionary money holdings should be associated, not only with a degree of ignorance on the part of agents, but also with markets characterised by a degree of price stickiness.

In a money using economy the extent of the gaps, between complete price flexibility and the amount which actually characterises markets, and between "all available" information and the amount gathered and utilised, will, moreover, vary. I am not only referring here to the possibility of exogenous technical progress in communications and data processing, though in a secular perspective these matters are surely important. I am also thinking of the endogenous responses of agents to variations in money's effectiveness in mitigating the consequences of ignorance and inflexibility. If money holding is a cheap and reliable buffer, then agents will find that it pays to remain

relatively uninformed about the processes affecting the variability of their net receipts, and will be relatively unwilling to undertake any costly measures that might render them either more predictable or controllable. If, on the other hand, money holding itself is a costly or unreliable source of insulation from such uncertainty, then the expenditure necessary to acquire and utilise extra information is more likely to be made.

Both the costs of using money as a buffer against market uncertainty and its reliability in that role, have to do with the behaviour of the general price level. As is well known, expected changes in money's purchasing power are an important component of the opportunity cost of holding it. If money holding's capacity to act as a buffer against costly consequences of market uncertainty and inflexibility is available only at a high price, agents have a strong incentive to minimise those costs by other means. Hence, the foregoing argument suggests that anticipated inflation both discourages money holding and encourages expenditure on information and the maintenance of price flexibility on the part of agents. From the private point of view, such a response is presumably optimal, but from a social perspective it is wasteful. It involves devoting real resources to the reduction of costs that would, in conditions of price stability, have been offset by holding money; and, so it is usual to argue, once we leave a commodity money world, real balances are, on the margin, socially cheap (indeed perhaps free) to provide.[12]

It is worth drawing explicit attention to the fact that the informational problems encountered in a monetary economy of the type I am discussing here are rather different from those met up with in a new-Classical system. There, sellers are presented *gratis* with all available information about their own market, in the shape of a given money price for their output,

but have to extract an estimate of the general price level (in order to compute a relative price) from other data. With endogenous price setting, the information most needed by agents concerns the state of the market in which they are dealing. Data on the general price level are not irrelevant here, but it is hard to believe that they are of primary importance relative to local factors. Hence the fact that agents apparently do not make use of readily available information about the money-supply, so puzzling to a new-Classical economist, is easily reconciled with the rationality postulate in an economy with endogenous price setting and information that is costly on the margin.[13]

However, the degree of priority given by agents to generating information about the general price level's behaviour will itself be endogenous. Anticipated changes in the price level influence the cost of obtaining money's services and unanticipated changes reduce the quality of those services; while, crucially, the extent to which price level variations are anticipated or unanticipated will itself depend upon the resources agents devote to gathering data on the matter and analysing them. Fluctuations in the price level thus create incentives to acquire expertise not just about local market conditions but also about the monetary system. The larger those fluctuations, the more productive (privately) does it become to gather the latter kind of information and to act upon it.

There is, though, an externality here. Monetary instability encourages individual agents to acquire information and to gain flexibility, so that they may vary their own prices to counter its effects. Such price flexibility tends to be self generating however, because the more do other prices vary, the more does it pay the individual dealer to maintain and utilise the

capacity to vary his own. To give one example of this phenomenon, Holland (1988) has shown, over the period 1965-1986, that the degree of wage flexibility in the U.S. has systematically increased in response to the intensity of price level fluctuations. At the level of the economy as a whole, these tendencies reduce money's usefulness, and so, as Keynes (1936, p. 269) long ago noted, price flexibility may not be quite the unmixed blessing that economists sometimes think it to be. One of its effects is to undermine money balances' capacity to act as a substitute for costly information gathering and processing.[14] It is perhaps not surprising that the political popularity of replacing decentralised and largely unregulated monetary exchange with alternative dirigiste schemes for co-ordinating economic activity tends to be greatest in periods of monetary instability.

SOME IMPLICATIONS FOR MACRO-MODELLING

The foregoing analysis has implications for how we should model price formation mechanisms in general and expectations mechanisms in particular. The new-Classical assumption that economic agents understand the structure of the economy and use that understanding to make optimal forcasts of the variables which concern them was never intended to be taken literally. Rather its proponents would have us think of agents as operating by "rule of thumb", and discarding rules which systematically mislead them until they find one that works "as if" its forecasts were being generated by a correct model of the economy they inhabit.

But how close to a "correct model" need a rule of thumb be for agents to be satisfied with it? It would presumably suffice if its forecasts produced no systematic errors with consequences costly enough to make it worth agents'

while to devote resources to reducing them. Economies in which money is cheap to hold and stable in its purchasing power give fewer incentives for simple and potentially erroneous rules to be weeded out. They are, therefore, likely to be be inhabited by agents whose "rational expectations" about the price level are subject to a higher variance, and are less robust in the face of changes in the pattern of shocks impinging on the economy, than those with a history of inflation and monetary instability. A simple example (based on the assumption that monetarist analysis is broadly correct) will illustrate some possible implications of such a view.

In a small open economy operating a fixed exchange rate on a world economy characterised by price level stability, the rule of thumb that tomorrow's price level will be close to its average value over some past period will give essentially the same prediction as one that has it moving in proportion to the nominal-money-real-GNP ratio, or indeed as the forecast of some more complex "true" model of the economy. An economist modelling such an economy would have equal predictive success by attributing to its inhabitants any of the above means of forecasting price level behaviour. If those inhabitants were using the first rule of thumb, however, they would be ill equipped to cope with the adoption of flexible exchange rates and unstable discretionary monetary policy. They might discover a more general rule in due course, but, as Parkin's (1977) analysis suggests, the economist modelling the transition between regimes would be in trouble if he ignored the learning process and simply attributed his version of the "true" model to them at all times. At the same time, if agents did learn to monitor the money supply, and as a consequence adopted a rule of thumb a step closer to the "true" model, then they would have little problem in forming expectations relevant to a

transition back to a fixed rate or some similar stable regime, nor would an economist predicting their behaviour.

It is a mistake to treat "expectations" as purely psychological phenomena when engaged in macro-modelling. What proximately matters for the way economies function is not what people expect, but how they act, and agents who have new information, understand its significance, but are not free to act upon it, may as well be ignorant. Nominal rigidities may be the result of desirable resource economising arrangements in an economy in which cash balances efficiently and cheaply buffer agents against the consequences of any imbalances in money flows which those rigidities may provoke; but they can cause real problems if circumstances change so as to reduce money's capacity to play that role. To return to the previous illustration, under a fixed exchange rate the success of the simple rule of thumb that the price level will tend to maintain a stable average level will encourage agents to engage in fixed nominal price contracts for significant time periods. Moreover the length and rigidity of those contracts will have more to do with uncertainties about conditions in the market for the particular item being traded, than with the likely course of the purchasing power of money. Under a discretionary monetary regime, when price level behaviour becomes an issue, we might expect to find shorter contracts and more indexing, even though these involve more transactions costs. Moreover the transition from the fixed rate to discretionary policy would likely be more difficult than the reverse change, because of this contrast in institutional starting points.

Now I do not intend the particular piece of elementary conceptual history running through the preceding paragraphs to be taken literally. I do, however, intend to be taken seriously the general message which it

illustrates: namely that it is dangerous uncritically to model agents' behaviour on the assumption that they form their expectations using the same model of their economy as the economist who studies them. Maximising agents ought not always to be expected to act like that. Rather they will utilise only the information which the history of their economy has provided them with an incentive to acquire. Properly understood, however, this argument does not lead to the conclusion that new-Classical analysis is always and everywhere misleading. Economies in which agents' rules of thumb are consistent with the processes driving the behaviour of critical variables will look very much as if they are inhabited by well informed rational agents of the textbook variety. Those rules of thumb are likely to be rather sophisticated in economies where the incentives to develop them have been strong, and the institutional flexibility needed for agents to respond quickly to new information is also likely to be present in such economies. Furthermore, ignorance and nominal rigidities, which characterise economies with an history of monetary stability, will be quite irrelevant to predicting their behaviour so long as that monetary stability prevails. New-Classical models should cope rather well with data generated by either type of economy.

Our analysis also warns us, however, that models which postulate that agents act "as if" they understand the structure of the economy are unlikely to work well during transition periods, particularly those which involve a reduction in money's capacity to provide a cheap hedge against ignorance and inflexibility. If agents' rules of thumb mimic in their predictions the economy's true structure only in a particular policy environment, but create systematic errors in another, then agents will have to experience those errors and the costs they impose before they can be expected to begin to correct

them. Far from being ruled out by the postulate of rational behaviour therefore, systematic error would seem to be required by its application to learning processes. Hence, instead of refusing to take seriously explanations of particular historical episodes which rely on systematic errors on the part of agents, or price rigidities that are "irrational" in prevailing circumstances, we should look askance at any account of the onset of instability that does not pay due attention to these phenomena. Moreover, because the period over which any individual is in a position to take important economic decisions (about such things as pricing policies, for example) is a good deal shorter than a lifetime, and because the intergenerational transmission of knowledge is an uncertain process, we should not rule out the possibility of "errors" being repeated either. A good deal of older work on business cycle history, and related topics, might regain respectability from this observation.

Now though the idea that old rules of thumb and institutional arrangements persist in new environments with which they are incompatible might help us understand the earlier stages of episodes of monetary instability, we must also recognise that the later stages will surely be characterised by the discovery and adoption of new ones. It is much easier to draw attention to the problem of how to enbed an account of learning in an economic model than to solve it. Let me simply observe here that attempts to model agents' learning processes "as if" they were econometricians seeking information about the economy by statistical induction seem to me to be of limited usefulness. Many of my colleagues would probably be delighted to see me proven wrong here, because if we could formulate an empirically robust theory along such lines, it would enable us to use the postulate of rational

maximising behaviour to cope with transition periods as well as with times of stability and hence to extend the area of experience to which new-Classical analysis can usefully be applied.[15]

There are two inter-related problems here. First, on my reading of the history of our discipline, economic knowledge seems to evolve by a process much more akin to that which Karl Popper characterised as "conjecture and refutation" than any other; and second, there is an inherently creative and hence unpredictable element to the process of conjecturing hypotheses about how the economy operates.[16] It is only if we envisage an end to the development of *qualitative* economic knowledge, so that learning need only be concerned with *quantitative* matters, that techniques of statistical inference will enable us to model all relevant aspects of learning satisfactorily. However, there is considerable comfort to be found in this, at first sight, nihilistic observation. Rymes (1979) and, more recently, Sargent (1984) have drawn our attention to the fact that it is all too easy to be led into an essentially vacuous type of economic determinism if we push ideas of rationality and foresight too far. To introduce an inherently unpredictable element into our vision of economic life, in the shape of a postulate about how learning takes place, enables us to avoid this particular trap. Perhaps it is the social scientist who believes that everything can and must be explained as the outcome of carefully calculated rational behaviour who is the real nihilist in this instance.[17]

PREDICTIONS ABOUT THE DEMAND FOR MONEY

Now the vision of market mechanisms underlying the foregoing arguments focusses on money's means of exchange function and argues that money holding should be viewed as an essentially precautionary phenomenon. To the extent that this rather specific view of money's role in economic life gets support from empirical work, those arguments are strengthened and begin to lose their predominantly *a priori* character. Empirical work on the aggregate demand for money function began in earnest in the late 1950s as a result of a change in the basic question which monetary economists asked. Instead of "why do people hold money?" the issue became "given that they do, what determines how much they will hold?", and Friedman's (1956) bold postulate that the aggregate demand for money was a stable function of but a few arguments was the starting point in settling it. The "money-as-if-a-consumer-durable" approach adopted in much empirical work yielded large dividends in quantitative knowledge by modelling the aggregate demand for real balances function as the sum of the demand functions of individual agents who treated money as a source of purely private utility yielding services. It ignored the nature of those services, not because they had not been analysed, but because the empirical questions being asked did not seem to require that attention be paid to them.

Even so, it was soon pointed out, notably by Fried (1973), Dutton and Gramm (1973), Karni (1974), Diewert (1974), and Phlips (1978) that models of the demand for money which incorporate brokerage fees, as precautionary models inevitably do, may rather easily be made to yield the prediction that the demand for money should depend upon the real wage level. The key step here is to characterise this "fee" as representing the time that agents must spend to turn other assets into cash.[18] This prediction was confirmed by evidence

presented in the last four of the above-mentioned studies, and has more recently been confirmed again by Dowd (1985). Also, as Fried stressed, when the prediction in question is combined with the observation that, at the level of the economy as whole, real income and real wages tend to move together, it implies that regressions which include only the former variable will yield an upward biased estimate of its coefficient. This argument is important because it reconciles the characteristic prediction of precautionary demand models that the income elasticity of demand for money is below unity with the frequent occurence of empirical estimates of this parameter in the region of unity.[19]

Evidence that it pays empirically to take explicit note of money's means of exchange function has thus long been available, but while it seemed possible to obtain good empirical performance from simpler functions which ignored this detail, this evidence had little impact. We can no longer be confident that it is safe to ignore the role money plays in the economy when we model the demand for it. Problems of empirical stability are neither illusory, nor confined to recent data. Closer inspection of already well analysed historical time series has revealed shifts in what initially seemed to be stable relationships. Recent work on these matters has exploited ideas about money that stress its means of exchange role. Thus, Bordo and Jonung (1987), invoking the essentially Classical analysis of Knut Wicksell (1906) as a starting point, have investigated the effects of such institutional factors as better communications, easier access to banking facilities, and so on, on the demand for money, and have found them to be systematically important. Such explanations of specific shifts in the demand for money function seem both plausible and natural in the light of the arguments presented earlier.

Bordo and Jonung's work thus gives us further cause to treat money's means of exchange role, and the closely related precautionary nature of money holding, as being of considerable empirical relevance.

I noted earlier in this lecture that the dynamics of the "short run demand for money function" may be derived from postulates about the degrees of price stickiness and expectational sophistication prevalent in the economy. Thus the precautionary approach to modelling the demand for money enables us to explain this aspect of the empirical evidence without resort to postulates about portfolio adjustment costs or measurement error, but as a natural characteristic of a monetary economy. Furthermore, in this view, short run instability in velocity can be explained as the consequence of variations in price stickiness and informational characteristics, both between economies and over time. Such an approach to this important empirical problem once more follows immediately from the vision of money's role in the economy advanced above, a vision which has proved to be a fruitful foundation for empirical work on the demand for money.

The work in question, usually referred to as "buffer stock modelling", is not, of course, without its critics. Thus Milbourne (1988) has recently prepared a careful survey of much of the literature to which I am here alluding, and points to three types of problem. First, he notes, correctly, that some models rely on the dubious assumption that nominal money is a completely exogenous variable to generate their predictions. The assumption in question is certainly helpful to exposition, but fortunately it is not essential, as work by, for example, Gordon (1984) shows. Second he argues that there are severe econometric problems with attempts to test buffer-stock ideas using single equation techniques. Not everyone (eg. Carr, Darby, and

Thornton (1985)) accepts this criticism, but that work which follows the lead of Bergstrom and Wymer (1974) and Jonson, Moses and Wymer (1976) in taking a complete model approach to estimation is in any event immune to it. Finally Milbourne notes, citing Cuthbertson and Taylor (1986), that, when it is combined with the new-Classical version of the rational expectations idea, which treats information and data processing as free goods, the "buffer-stock" approach runs into real empirical difficulties because the data appear to reject the restrictions on parameter values implied by the resulting model. However, more recent work by Cuthbertson and Taylor (1987), which was not available to Milbourne, shows that this problem may be dealt with by attributing what we might term "sensible" rather than fully rational expectations to agents. The arguments of this lecture strongly suggest that the existence of a significant precautionary element in the demand for money is likely to be incompatible with any extreme categorisation of expectations, and hence gain support from these more recent results.[20]

A number of other predictions about the demand for money follow from the ideas set out in this lecture. For example they imply that we should not think of the individual's precautionary demand for money as being underpinned by a stochastic pattern of payments and receipts generated by some exogenously given structure. Far from being exogenously given, that stochastic pattern is, if only within limits, controllable. Its dispersion can be reduced by devoting more resources to gathering market information, and by devoting more resources to maintaining the capacity to act flexibly in the market. Moreover, the incentive to devote resources to these ends varies systematically with the cost of holding money. When this cost is high, resources will be devoted to reducing the dispersion of cash flows, and vice

versa. Hence *movements along* a demand for precautionary balances function derived on the assumption of a given structure of information will be accompanied by systematic *shifts of* that function. Note also that those shifts will not be reversible to the extent that information, once acquired, or new contractual arrangements once put in place, are durable. Ratchet effects in the demand for money-interest rate relationship, which figured prominently in the empirical work on the alleged instability of the U.S. demand for money function surveyed by Judd and Scadding (1982), might repay further investigation in the light of this argument.

The foregoing analysis also suggests that complications, in the form of interdependencies among agents' money holdings, might arise in getting from the individual to the market demand curve for money. Such interdependencies will exist in principle because a time and trouble cost of obtaining cash in exchange for other assets is basic to any model of the individual's precautionary demand for money. The cost to any one agent, however, of obtaining cash will surely vary with the amount of cash which other inhabitants of the economy hold. The more money held by others, the lower will be, to any particular agent, the costs of finding a willing cash buyer for some other asset, and, therefore, the less money will that agent hold. Thus, as the nominal interest rate rises, and each individual reduces cash holdings, the fact that all other agents simultaneously do the same produces forces which offset, though not of course entirely, this negative movement. The aggregate demand for money function will, then, be more steeply sloped with respect to the rate of interest than the sum of the individual relationships.[21]

Now this last prediction can hardly be tested directly. To do so we would need data in which the opportunity cost of holding money varied for an individual while it remained constant for the rest of the economy. However it might be indirectly tested by noting that cross-section data on the demand for money as a function of real income tell us something about how an individual's money holdings vary when those of all other agents remain constant, while aggregate time series evidence on the same relationship tells us about the same relationship when the money holdings of others are simultaneously changing. The externality just discussed would be held constant in the former case and at work in the latter, so the cross section income elasticity of demand should be the higher of the two. The general presumption about income effects is that cross section studies yield lower parameter estimates, so it is not without interest that the only systematic survey of evidence on this question of which I am aware (Feige and Pearce 1977) does in fact find cross section estimates to be higher on average. The difference here is not statistically significant and the work surveyed did not pay attention to brokerage fee effects, so one should not make too much of this result. Even so the question is surely worth more detailed examination than it has so far received.

CONCLUSION

Now it can be argued that the predictions about the demand for money function discussed above, both those which have been tested and confirmed, and those which for the moment remain conjectures, are of rather minor importance. So indeed they are as far as the demand function itself is concerned. They represent at best rather small embellishments to our

knowledge of this relationship. However, I have taken the time to discuss them here, not because of what they tell us about the appropriate empirical specification of the demand for money function *per se*, but because of what their empirical confirmation suggests, or would suggest, about the motives underlying the behaviour which that function summarises.

As I have remarked above, where trading at false, and perhaps sticky, prices set on the basis of less than "all available" information is of the essence, serially correlated fluctuations in output and employment, not to mention prices and real balances, naturally arise. In such a world, money is significantly non-neutral, and monetary policy is important. Though it was at one time taken for granted that any satisfactory macroeconomic model would display such characteristics, the recent fashion for building upon Walrasian microfoundations has undermined any consensus about such matters. I have argued in this lecture that an alternative, non-Walrasian, vision of the workings of the economy can be used to defend a traditional approach to macroeconomic modelling, and that a precautionary view of money holding is central to that vision. Thus, if evidence that the precautionary model of the demand for money has explanatory power over real world data makes a trivial difference to our beliefs about the properties of the demand for money function, it ought nevertheless to have a profound effect on the way we think about the workings of market economies. That is the most important reason of all for taking money seriously.

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FOOTNOTES

* The 1988 Presidential Address to the Canadian Economics Association, delivered in Windsor, Ontario on June 4, 1988. During the past year I have had fruitful discussions with Karl Brunner, Axel Leijonhufvud, Bennett McCallum, Allan Meltzer, Edmund Phelps, Thomas Sargent and John Taylor about many of the issues dealt with here, and owe a special debt to Joel Fried and Peter Howitt for reading and commenting in great detail on earlier drafts of this lecture. I have also benefitted from the comments of Paul Anglin, Keith Cuthbertson, Steve Holland, Seppo Honkapohja, Brian Loasby, Michael Parkin, Tom Rymes, and George Stadler. It should be more than usually apparent, however, that harmonious though these discussions were, they did not always end in agreement, and none of the above should be held responsible for the views which I express.

1. The parallels between the Keynesian Revolution and the Monetarist Counter-Revolution were analysed by Johnson (1971) in a lecture whose occasionally ad-hominem character should not to distract its reader's attention from its underlying seriousness. The standard reference on the distinction between "Keynesian Economics" and "The Economics of Keynes" is Leijonhufvud (1968).

2. The publication of the first edition of Patinkin's *Money, Interest and Prices* in 1956 was of course a major landmark here.

3. Hall cites papers by Black (1970) and Fama (1980) as being key contributions here, but the contributions to this literature which have attracted most attention are probably those of Wallace (1981) and

Sargent and Wallace (1983). Note that Bennett McCallum (1983) criticises this stream of the literature, particularly that part of it which explicitly relies on Samuelson's (1958) "overlapping generations" framework, for neglecting money's means of exchange role, and in (1987) shows how adapting an "overlapping generations" model to accomodate this phenonemon changes its properties. Wallace (1988) provides a non-technical survey of the "legal restrictions" theory of money which remains useful despite the fact that it systematically ignores any work that is critical of the approach.

4. Thus Barro's (1978) work, which seemed to confirm new-Classical predictions about the importance of "unanticipated money" and the irrelevance of "anticipated money" for real economic variables is now regarded with considerable scepticism mainly as a result of the work of Mishkin (1982) and Boschen and Grossman (1982). Note that another reason for the current popularity of real business cycle theory is the finding of Nelson and Plosser (1982) that the time path of United States real income is satisfactorily characterised as a random walk. This finding, however, seems fragile, if recent work by John McCallum (1988) and Pierre Perron (1987) is taken into account.

5. The data utilised in figure 1 are for the following North American and European countries and time periods: U. S. A. 1971-85, Germany 1974-85, Canada 1970-84, U.K. 1970-84, Italy 1971-84, Japan 1971-84, Sweden 1971-85, Spain 1971-83, Switzerland 1970-85, Belgium 1970-84, and the Netherlands 1971-84. France is omitted because of a major break in its money supply series. In each case the money supply is "narrow" money, real output is GNP, and the price level is the GNP deflator. Each

scatter has plotted with it the relevant least squares regression line, with the standard error of its coefficients given below them in parentheses. The possibility that the money-growth-output relationship is the result of "reverse-causation" can never be completely ruled out, though the plausibility of this explanation is reduced by the time lag inherent in figure 1b. Historical analysis of particular episodes, such as that employed by Friedman and Schwartz (1963) on the Great Depression, or Howitt (1986) on more recent Canadian experience seems to me to provide the best means of countering this line of argument.

6. Parkin (1987) has reported empirical work based on data related to, but not identical with, those I have used here. He claims his results to be inconsistent with a "sticky-price Keynesian" view of cyclical fluctuations, but consistent with a New-Classical view. To accept Parkin's interpretation of the data requires us to: (a) treat changes in relative commodity prices as reflecting solely the influence of productivity and taste shocks, and as occurring independently of demand side disturbances; (b) ignore the work of Boschen and Grossman (1982) and accept that maximising agents are unable to use information accruing during a year to influence their behaviour until the subsequent year; and (c) agree that sticky price models always yield the implications that monetary innovations "Granger-cause" (i.e. unambiguously lead) innovations in real variables in annual data. Though it might be sensible to take any of these steps in the face of particular data sets, and in the context of specific models, there is no general reason to do so. Thus, Parkin's interpretation of his results claims more for them than they can really support.

7. Brian Loasby has drawn my attention to the strong similarities between such a treatment of money, and Coase's celebrated (1937) analysis of the firm as a social institution. To discuss this insight here is beyond the scope of this essay, but the parallel is well worth following up.

8. An economy which only partially dispenses with the auctioneer's role is nevertheless a particularly useful fiction in order to simplify the modelling of the evolution of a means of exchange from an initial condition of barter. Most attempts to find a microfoundation for money are content to explain what it does, without concerning themselves about how it comes into being. Jones (1976) and, more recently, Kiyotaki and Wright (1987) are notable and important exceptions here. The latter paper is of particular interest in the current context because, in the model it analyses, dealers and monetary exchange emerge simultaneously as complementary phenomena.

9. The analysis of the preceding two paragraphs draws rather heavily on arguments which I set out in Laidler (1974). The reader's attention is drawn to papers by Leijonhufvud (1973) and Jonson (1976) which also explore these and related aspects of money's "buffer-stock" role.

10. A particularly simple version of such a model was developed by Weinrobe (1972), and forms the basis of my own exposition of the precautionary motive in my book on the *The Demand for Money*. . . (Laidler 1985) where the reader will also find references to other, more complex, treatments of this topic.

11. And when we put matters this way, it becomes clear that we are dealing with ideas first advanced in Stigler's seminal (1961) paper on the "Economics of Information" and discussed in the context of monetary

economics by, among others, Brunner and Meltzer (1971) and Laidler (1974). Brunner and Meltzer have recently taken up this theme again in (1987) and develop it along lines very similar to those I am following here.

12. Anyone who has participated in informal discussions about recent inflationary episodes in such countries as Argentina or Israel will have heard anecdotes about the sophistication which their ordinary inhabitants develop about market processes. Instead of admiring this sophistication, as we so often seem to, we might equally well deplore it as a manifestation of resource misallocation induced by irresponsible policy.

13. Thus, anyone seeking a microeconomic story about inflation-unemployment-output interaction to complement the monetary economics I am here expounding should look to the work of Phelps (1969, 1972) as a starting point, rather than to the "aggregate supply curve" analysis of new-Classical theory.

14. Both positive and normative aspects of these matters deserve a great deal more attention than I have space to give them here. Klein (1977) argued that price level uncertainty could cause the demand for money either to rise or fall, depending upon whether agents decided to hold more or less money in the face of a fall in the volume of services produced by a unit of it. His empirical evidence seemed to show a mild positive relationship here, but subsequent work by Laidler (1980) showed that data for the 1970s reversed this result. More recently Friedman (1984) has conjectured that instability in the rate of money growth (which is related but by no means identical to price level uncertainty)

ought to lead to a higher demand for money, a conjecture that receives some support from evidence recently published by Hall and Noble (1987). I conjecture that monetary instability, to the extent that it leads to fluctuating expnditure flows will increase the demand for money, and, to the extent that it increases price level uncertainty, will reduce it. I also conjecture that Klein's ambiguous prediction about the latter relationship arises from his having used a model in which there exists no substitute for "money services". If such a substitute does exist, in the form of resources devoted to information generation, it seems to me more likely that a reduction in money's capacity to perform its services will lead to a reduction in the demand for it. Finally, I suspect that the normative significance of fluctuations whose effects may be offset by increased money holdings will, upon analysis, prove to be quite different from that to be attached to instability that undermines money's capacity to function as an information substitute.

15. Recent examples of this type of analysis include Marcet and Sargent (1987) and Howitt (1987).

16. I do not wish to be thought of as a supporter of a naive view of scientific method that has the individual economist conjecturing new ideas and then disinterestedly seeking to refute them. The process here is a social one, in which self-interested individuals try as hard as they can to refute the conjectures of their competitors, while simultaneously defending their own.

17. An emphasis on the limits of rationality as a foundation for economic analysis is a pervasive theme of "post-Keynesian" macroeconomics. See Chick (1983) and Foster (1987) for recent examples of work along these

lines. Note also that New-Classical economists are not ignoring this problem at least as it arises in the context of transitions in the state of knowledge. See for example Lucas (1986) for a penetrating discussion of the issues involved, a discussion in which, let it be emphasised, he shows himself to be extremely sympathetic to the notion that there are limits to the explanatory power of the rationality postulate.

18. Of course brokerage fees also play a prominent role in Baumol (1953) Tobin (1956) transactions demand models. Those models concentrate on money's means of exchange role, and hence are close to the spirit of the approach to monetary economics that underlies this lecture. Indeed, Clower and Howitt (1976) may be regarded as an exploration of how the Baumol-Tobin model should be modified in the context of a non-Walrasian economy.

19. Lucas (1988) has shown that one such function with a unit elasticity, namely that proposed and estimated by Meltzer (1963), still seems to be discernible in United States data, and he has also shown how such a relationship can be interpreted as part of the structure of an almost Walrasian (i.e market clearing prices, rational expectations, plus a cash in advance or credit constraint) economy. Hence it is important for any advocate of the approach to money taken in this paper to be able to reconcile such evidence with that view. Note that Svensson (1985) and Hartley (1988) introduce a precautionary element into the demand for money in a version of Lucas' system by altering the timing with which information is made available to agents and by permitting cash to be used in a wider (but overlapping) range of markets than credit. They thus attribute to cash the capacity to generate market "flexibility" of

a type first analysed by Goldman (1974). All this analysis treats the provision of information as exogenous, and hence differs crucially from the approach taken here, but it nevertheless represents a step towards closing the gap between Lucas's approach to the analysis of money and that advocated in this lecture.

20. Even so, the ideas under discussion here are difficult to test directly and definitively, because, as Milbourne's survey suggests, only a properly specified complete macro-model provides an appropriate vehicle for investigating them. There is obviously much room for debate about what such a model would look like, and hence considerable scope for creating alibis for particular results.

21. I discussed this externality in Laidler (1977). Note that it bears a strong family resemblence to the externalities in search processes that produce the multiple equilibria which characterise the models of Diamond (eg. 1984) and Howitt (1985), and which have been analysed more generally by Drazen (1987).

Figure 1a

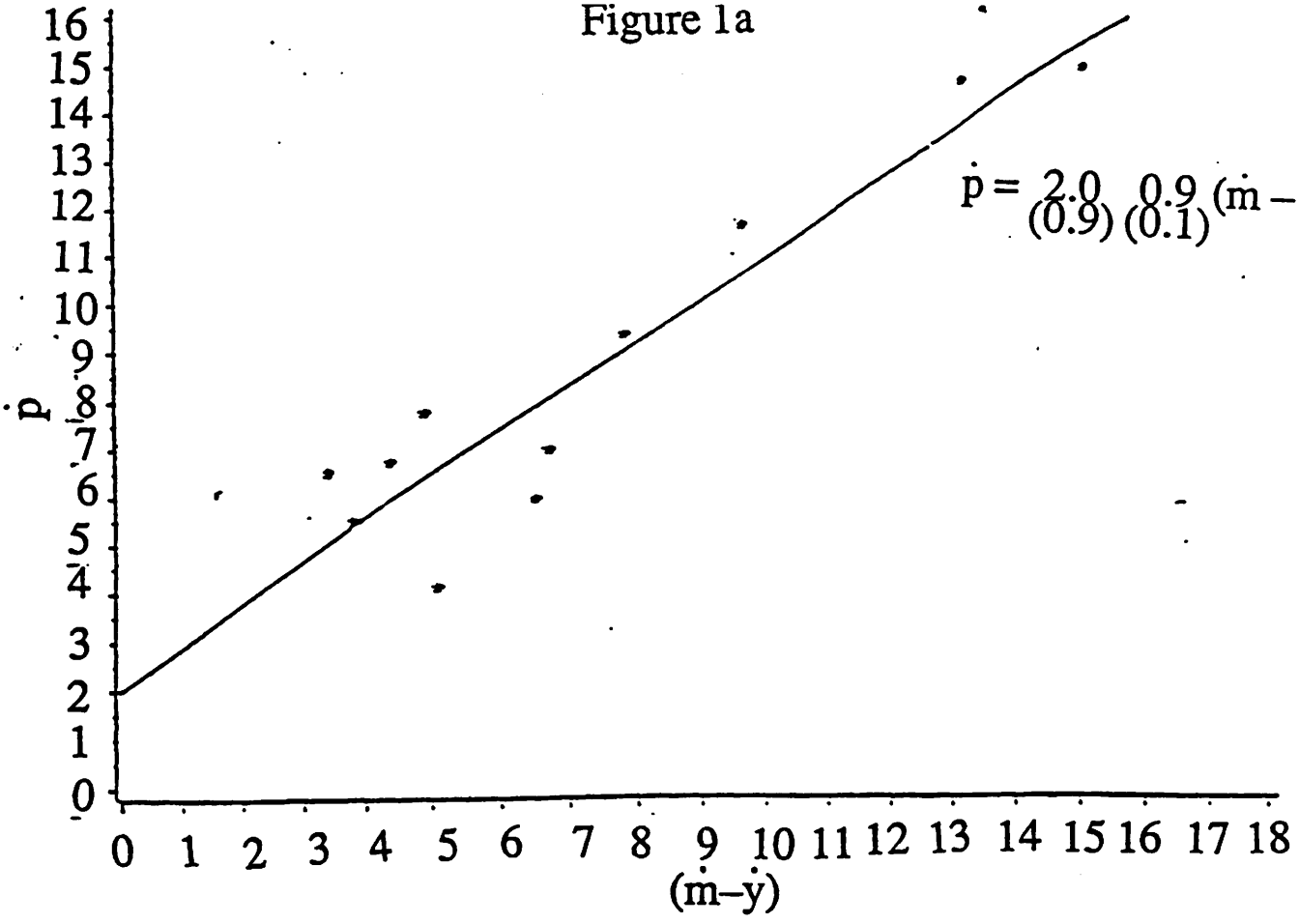

Figure 1b